July 28, 1948.

Dear Luria,

I know that you will be glad to hear that a heterozygote has been found in E. coli K-12. It should be emphasized that such a delay in the reduction of the zygote is quite exceptional (probably less than .01% of the recombinations) but once again, this exception may help to prove the rule.

V^r_{1c} is a very peculiar T1-resistant type. It is sensitive to T5, while T1 and T1h form "plaques" which are not lytic areas but regions which are recognizable as being peculiarly stained on EMB agar. On nutrient agar, one sees perhaps a faint inhibition of growth. Application of T1 to V^r_{1c} does not lead to the development of typical resistants, and V^r_{1c} is not lysogenic. At any rate, I was running through a routine test for allelism with the cross: $\frac{B_1- \quad Lac- \quad V_1{}^r \quad T- \; L-}{B- \; M- \quad (V_{1c}{}^r)}$,

testing the prototrophs for sensitivity to T1 and T5 on synthetic EMB. Out of more than 200 tests, only one culture was sensitive to T1. Since the distinction between the V_1 and V_{1c} loci would depend on this lone culture, it was purified and retested for sensitivity. All the cultures tested then were resistant. It turns out that the original prototroph (W-465) is sensitive on minimal agar, and is Lac+, but is continually throwing off pure true-breeding Lac- and Lac+ segregants on complete medium. The segregants are of the whole gamut of recombination types, including some (e.g. B-M-T-L-Lac-) which are unobtainable by the older selective method. The rate of segregation is so high that colonies on EMB Lac are thoroughly sectored or mosaic. A few with large sectors are under study to look for complementary segregants.

As you might imagine, I am worried about being able to keep this interesting culture, but frequent transfers with concomitant testing seem to be quite successful for the moment. Of course, some of the recombinants (segregants) are prototrophs, so that even keeping it on minimal agar is not certain to succeed. Lyophil may be the answer.

Of course, with this culture, it will be necessary to repeat the linkage determinations. So far, everything is as it should be, although my indirect estimate of the absolute distance between (BM) and (TL) may be somewhat high.

Of course single-cell tests would be far more significant here than they were previously (although the very high segregation rate is probably decisive by itself), and arrangements are being made for these as well as for cytological study. Just offhand, there was no particular sign of filamentous forms, in a simple smear.

So far everything points to the delay in reduction being an unpredictable accident, but now that it is clear what to look for, a closer scrutiny will be made.

As you requested, I sent a batch of cultures to Miss Kann quite some time ago, but they were mysteriously lost; however, she wrote that the second batch just sent have arrived.

Note the suggestion above that phage-resistance is recessive. I am quite convinved that the delayed effect is a segregation, probably of nuclei.

I am writing in such detail to you because I owe, in part, my attention to the possibility that W-465 was zygotic to thinking generated by your criticisms at Minneapolis.

With best regards,

Joshua Lederberg